Section 4

General Comments on PATH

by Carl Walters

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I have had a bit of time to reflect on the general directions and approaches that PATH is taking. I am concerned that the process is getting lost in detail, such that it will not provide a clear vision about how to proceed with experimental management in the Columbia Basin. Two things worry me especially: (1) there still does not appear to be general acceptance that future policy should be experimental in the first place; I still see a lot of grasping for "best" estimates and desire to define a single, permanent policy option to promote (e.g., irreversible alteration of dams); (2) there is confusion about what probability assessments are needed for planning: you refer to problems about computing "posterior" probabilities from the data, when talking about using prior probabilities in decision analyses, where those priors ought to be only over alternative hypotheses about response to policy changes that cannot be clearly discarded on the basis of historical data (i.e., ought to be assigned uniform priors, so there is no statistical issue to deal with in the first place).

At lunch today I stumbled on a quote by Lawrence Wilkinson in the peculiar magazine Wired, that pretty much says it all: "Scenario planning begins by identifying the focal issue or decision. There are an infinite number of stories that we could tell about the future; our purpose is to tell those that matter, that lead to better decisions." In your setting, this quote means: stop wringing your hands about which models to use and how to include all sorts of detailed uncertainties, and focus instead on defining the range of possible outcomes for a strategic set of experimental policy options. Put another way: successful adaptive management planning exercises have been characterized by people finding a vision of what the experimental policy regime ought to look like (sequence of flow regimes, transport treatment pattern over time and space, etc.), then gearing up whatever models are needed to evaluate whether this regime will work (will resolve uncertainties in sufficiently "safe" way) and whether it is relatively better than various non-adaptive (or practically irreversible) options. Having analysed a great deal of historical data, you can now say the data do not allow resolution of at least two basic uncertainties about policy (equal odds on a range of outcomes of change in these policies): delayed transportation effects, and impact of flow variation. You have a set of transportation and flow management options at hand (A1-C2, maximize transportation, flow augmentation, Snake drawdown, John Day drawdown), and at least some analysis of the practicality/reversibility of implementing each of these. That is a good start. But what is glaringly missing is a vision of how to link the options to the uncertainties, i.e., proposals for SEQUENTIAL application of options and monitoring of which one(s) work best. In other words, you have not defined ADAPTIVE management "options" at all; you have just defined what we call "actions" that can be packaged in various combinations over time to define experimental policy options.

I very strongly suspect that as soon as you attempt this packaging step, you will find that there are in fact only a few viable experimental policy options (treatment sequences are strongly constrained by availability and storage of water, need to avoid confounding of effects of alternative treatment types and also test for positive interaction effects like augmentation plus transportation outperforming predicted sum of effects of either by itself). In short, a shared vision about a few policy regimes worth evaluating in further detail will likely leap out at you when you begin to define sensible treatment sequences. When you move to develop formal decision analyses about how to compare your experimental "vision" option(s) to various baselines (A1 base case, etc.), be very careful not to get bogged down in how you define alternative hypotheses or states of nature for that decision analysis. There are millions of combinations of population and response parameter values that could each be treated as a "hypothesis"

about the dynamics. But all you should include in the decision analysis are alternative hypotheses about RESPONSE TO THE PARTICULAR POLICY OPTIONS. That is, your decision analysis hypotheses should be about response, not about population parameter values and patterns of stochastic variation. In general, each response hypothesis in such an analysis represents a whole bunch of hypotheses about the population parameter values. The population dynamics parameter hypotheses only matter in so far as they affect your assessment of how difficult it will be to measure the response to treatment, i.e., at what scales in both space and time to expect clear abundance responses to each treatment alternative.

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General Comments on the decision alternatives that PATH is analysing (paraphrased from discussion with Dave Marmorek, PATH Facilitator)

1. PATH should focus most of all on modelling the response to those decisions, rather than attempting to model the general behaviour of fish populations.

2. The historical data are not necessarily a good guide at all to what will happen in the future (similar to Randall=s comments).

3. The actions taken would be much more informative if they were taken in some kind of on-off sequence, so that the signal in fish populations could better resolve some of the remaining uncertainties. For example, a factorial design of manipulating transportation and flow, implemented in a pulsed sequence, would actually be much more informative than a permanent decision one way or another.

4. The general structure for how PATH is implementing drawdown was reasonable. There is no certainty whatsoever that predation would quickly decline to pre-Snake River dam levels, since the decrease in volume of water would increase the density of predators per unit volume. It is important that this process be considered in simulating their responses to drawdown.

5. On the issue of differential mortality of upstream vs. downstream stocks, there were some data on the survival of upriver hatchery chinook stocks in the Fraser River relative to downriver stocks. My vague recollection is that upstream stocks did have poorer survival than downstream stocks, partially related to operation problems at some of the upstream hatcheries.

6. On the issues of the alternative hypotheses that exist with respect to extra, there has been an observed progressive wave of declining marine survival moving northward from the Columbia River toward chinook and coho stocks on Vancouver Island. It might be possible that a combination of increases in temperature and increases in BKD could have some relationship to this pattern. As to why downstream stocks with high rates of BKD would still be functioning at a higher rate of recruitment than upstream stocks, it is not inconceivable that there are stock specific differences in ocean behaviour, which might lead the downstream stocks to seek lower temperatures, and therefore experience less mortality from BKD (higher temperatures exacerbate the impact of BKD).

7. It is extremely important to clarify in simple terms the differences between alternative hypotheses regarding ocean survival. Specifically, both the alpha and delta models= estimate of marine survival should be shown for each stock, or each stock group, and graphed alongside the timing of implementation of the various dams. The implied marine survival assumptions of each model need to be made very clear, and carefully reviewed. I am concerned about >fishing expeditions= for correlations with oceanographic and climate indicators.

8. I strongly encourage adaptive management experiments. The in-river option (there is no letter destination for this, perhaps A2'=) is worth implementing in a pulsed sequence with transportation to assess the benefits of transportation more explicitly. In general, the group should focus a lot of effort on assessing what sequence of treatments would be most informative with respect to resolving the key uncertainties (i.e. those uncertainties which affect what ultimate permanent decision is made). PATH should use some of the models that we have to simulate the information that managers would receive in the future if one or another hypothesis were true. Simulations would be run with only the Aoperator@ knowing which hypothesis was operative, and measurement errors would be added to the simulations. Included in this simulation would be all of the key uncertainties, especially the temporal pattern of marine survival rates. We would then observe the error rate in different participants diagnosing which hypothesis was operating. One of the most useful types of information gleaned from such an exercise are the requests that observers make for other types of information C this indicates which data are most critical to collect and design into the experiment. These ideas are discussed in the AUse of gaming procedures in evaluation of management experiments@ in Canadian Journal of Fisheries and Aquatic Sciences, Vol. 51(12), pages 2705-2714. We could consider the error rate in diagnosing which hypotheses was correct as a performance measure of the ability of a given action to discriminate among competing alternative hypotheses.